led him to modify his views in several ways. As it is, his preface is dated from Faizabad, and in it he describes himself as "a solitary Indian, far away from contact with men of science, but fresh from the actual and impressive presence of 'Nature's children.'" These words account for the freshness and vigour of his style, but they must not be taken to imply that his examination was made without want of knowledge of anthropology. So far from this, one of the great excellencies of the volume lies in showing how much more deeply an observer sees into the life of an uncivilised people, when he is engaged in examining evidence for and against current ethnological theories, than when he goes as a mere traveller, setting down at random anything that takes his attention.

EDWARD B. TYLOR

OUR BOOK SHELF

An Elementary Treatise on Geometrical Conic Sections. By G. Richardson, M.A. (Rivington, 1873.)

THIS is one of the volumes of the publisher's Mathematical Series, is very well printed, and has, if we are not mistaken, only three trivial misprints. There is quite a run at the present time on this subject, if we may judge by the number of treatises which have recently made their appearance, and this we are not altogether surprised at, as it is one of great interest; its theorems have great intrinsic beauty and almost boundless applications. The ordinary propositions are discussed not altogether in the usual order of consecution from the locus-point of view (the last chapter of four pages being devoted to the cone); the demonstrations are neat, and two or three are exceedingly concise as well. The only or chief novelty is the simultaneous treatment of the ellipse and the hyperbola, the corresponding propositions facing one another on the even and odd pages respectively. The discussion of the asymptotic properties of the latter curve pairs off against a series of propositions on projections. The book is a good working one for beginners, and embraces sufficient for the preliminary examination for mathematical honours at Cambridge, without having too much for school use. There is an extensive selection of exercises.

R. T.

Waste Products and Undeveloped Substances. A Synopsis of progress made in their economic utilisation during the last quarter of a century, at home and abroad. By P. L. Simmonds. (London: Hardwicke, 1873.)

MR. SIMMONDS'S book is seasonable in these days, when so much has been done in the utilisation of waste, as showing how very much yet remains to do.

In nearly 500 pages of close print he has drawn attention to a mass of matter almost bewildering in its vastness, and extending to nearly every kind of material in use in civilised communities. We cannot help noticing that Mr. Simmonds has been affected by the mass of subjects he has attempted, for the book very frequently displays a considerable lack of arrangement.

The author should look to this in a future edition, in which also the book might be easily and advantageously condensed to a considerable extent.

We must, however, thank the author for the service he does in calling the attention of civilisation to the extravagant, and we might say, "riotous" living with which its substance is wasted.

La Botanique de la Bible. Étude scientifique, historique, litteraire et exégétique des plantes mentionnées dans la Sainte-Ecriture. Par Frédéric Hamilton. 8vo. pp. 220, 25 photographs. (Nice: Eugène Fleurdelys, 1871.)

THIS interesting volume will possibly be unknown to the majority of our readers, and yet we venture to think that, from the beauty of its illustrations and the pleasantness of its style, it may to some of them prove a welcome addition to their knowledge of the subject on which it treats. Not stopping to discuss the nature of those mysterious trees said to have existed in the Garden of Eden, the author divides his subject into two parts. The first treating of the genera and species of which there can be little doubt, such as the pomegranate, almond, cedar, fig, &c.; and the second of those plants or portions of plants about which it is difficult to decide to what genus even they may belong, such as shittim-wood, hyssop, &c. In the first portion of the volume not only are the scientific characters of the plants given, but there is also added a series of references to them from the classics. The photographs are taken from living specimens growing chiefly in the neighbourhood of Nice and Mentone.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Effects of Temperature on Reflex Action

I DO not know if I quite understand Mr. Lewes's objections to my little article in the *Journal of Anatomy and Physiology.* He attributes the absence of movements in the case in question to a loss of sensibility *to temperature.* At first his statement reads as if the loss of sensibility to temperature were due to the removal of the brain. But he cannot mean this, because the whole of my paper starts from the fact that when the toes alone are exposed to gradually heated water, the leg is withdrawn. If he means that the sensibility to temperature alone is destroyed or depressed by the exposure of the whole body to the gradually heated water, and the other "sensibilities" left intact, I do not see how my argument touching the difference between the entire and the brainless frog is affected at all by a limitation of the stimulus to one particular kind. Moreover, in the last observation recorded in my paper it is expressly stated that in the later stages of heating the absence or diminution of reaction towards chemical as well as thermal stimuli was observed. Gradually heated water acts as a very slight stimulus, sulphuric acid (even dilute) is a very strong stimulus; and that the latter suddenly applied, as in the experiment of Goltz referred to by Mr. Lewes, should call forth a reflex action at a time when the former is unable to do so, in no way contradicts my explanation of the absence of movements. A red-hot iron might have been substituted for the sulphuric acid with identical results.

The paper in question had for its object simply the solution of the difficulty why the brainless frog allowed himself to be boiled without moving. In it I carefully avoided entering upon any discussion concerning Sensation (or Consciousness) in the spinal cord. The words "movement of volition, *that is*, a *movement carried out by the encephalon*,"—"ordinary reflex action, that is, a movement *carried out by the spinal cord alone*," were purposely chosen. I went so far as to speak of an "intelligent frog" and an "unintelligent reflex action," because we have means of measuring intelligence, and we can speak of a body as being conscious and yet not intelligent. I imagine that if Mr. Lewes and myself were to talk over the matter quietly, he would find that I am not so much at variance with him as he imagines. I feel with him the difficulty of refusing to the protoplasm of a white blood corpuscle, a something which may be evolved into (not out of) consciousness. That and like difficulties are not a little increased if, as Mr. Darwin seems to suggest, we regard inherited voluntary acts as the chief instead of the occasional source of reflex actions. Without entering into any long discussion, perhaps I may be permitted to say that in such matters as the movements of a brainless frog, it seems to me there are two things which ought to be kept separate: the investigation into the laws according to which those movements take place, *i.e.*, the study of the various nervous mechanisms of the spinal cord, and the question whether those movements, whether the working of those mechanisms, is or is not accompanied by consciousness. As a physiologist I am prepared to busy myself with the first, as I see prospects of success. With regard to the second, I am not prepared to say anything until we have ob-

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tained some better tokens of consciousness than the greater or less resemblance of the movements in question to such movements as our conscious selves are in the habit of executing.

M. FOSTER

Meyer's Exploration of New Guinea

FEW persons can have read Dr. Meyer's account of his recent adventurous and very successful journey with more interest than myself; but I confess I was surprised to find that the translator of my book should have misunderstood what I had stated, and so create a difference between us where none exists. He says (speaking of Dorey) that I "have not given a correct impression of the natives of the surrounding hills and mountains, separating them in some way from the inhabitants of the coast, as smaller, uglier, not mop-headed," &c.; and that he finds on the other hand, that "there is no generic difference at all between the Papooas of the mountain and the Papooas of the coast, except such differences as we find everywhere between the highlanders and coast inhabitants of the same race." Now I say exactly the same thing: "From these (sketches) and the captain's description, it appeared that the people of Arfak were similar to those of Dorey." ("Malay Archipelago," 3rd Ed. p. 505.) Dr. Meyer however, probably refers to what I say of the people of one hill village, close to Dorey: "The inhabitants seemed rather uglier than those at Dorey village. They are, no doubt, the true indigenes of this part of New Guinea, living in the interior, and subsisting by cultivation and hunting. The Dorey-men, on the other hand, are shore dwellers, fishers, and traders in a small way, and have thus the character of a colony who have migrated from another district. These hillmen, or Arfaks, differed much in physical features. They were generally black, but some were brown like Malays. Their hair, though always more or less frizzly, was sometimes short and matted," &c. (p. 499). I can only suppose that the word "differed" in the above passage was taken to mean "differed from the Dorey people," whereas the context shows that it means "differed among themselves," or varied, which would have been a better word. In the preceding page I have stated of the inhabitants of Dorey: "The majority have short woolly hair;" so that there is no difference from them in that respect. In all I have written about the Papuans I have maintained that the people of New Guinea and of all the immediately surrounding islands are of one race, with very unimportant local differences; and I do not think my remark, that the people of one village were "*rather* uglier" than those of another, three miles off, justifies the idea that I supposed there was any "difference," in an ethnological sense, between them. I cannot find that I have said a word about difference of stature.

The great success of both Messrs. D'Albertis and Meyer in penetrating inland in New Guinea will, it is to be hoped, induce other travellers to attempt the exploration of the far larger and less known southern portion. Two Europeans, with a small steam launch and a Malay crew, would, no doubt, be able to penetrate a long way up some of the larger rivers, and establish a station from which exploration of the central mountains might be effected. There is now no portion of the globe so completely unknown as this, or which promises such great results for every branch of Natural History.

ALFRED R. WALLACE

Deep-sea Sounding and Deep-sea Thermometers

WITH reference to the discussion which has recently been carried on in NATURE as to the deep-sea thermometers, I hope that perhaps the following statement may tend to put the matter at rest.

One of Negretti's thermometers was exhibited at the Royal United Service Institution at a lecture, March 11, 1859, by Admiral FitzRoy, who then spoke of them "as thermometers peculiarly constructed, self-registering," &c. The construction of these thermometers had been fully described in the "First number of Meteorological Papers, 1857," and was subsequently given in a "Treatise on Meteorological Instruments," published by Negretti and Zambra in 1864. The peculiarity of these thermometers was mentioned in the Hydrographic Instructions to Captain Dayman of the *Cyclops* Sounding Expedition, dated May 29, 1857. These facts are sufficient to show the ample publication of the device in question for protecting the bulbs against pressure.

I know from Dr. Miller himself that he did not know of Negretti's plan. In his paper in the Royal Society Proceedings, he calls the one which he describes a "simple expedient." I am not aware of any just claim on the part of Mr. Casella to the principle of the invention.

I consider that the practice of instrument makers designating by their names instruments which they have not *invented*, is most reprehensible.

ROBERT H. SCOTT

London, Dec. 9

[We have received a letter on this subject also from Mr. Casella, but as there is nothing in it bearing on the real point at issue, we do not print it. The above letter from Mr. Scott renders it clear to us, and it will doubtless be also clear to our readers, that the whole credit of the double bulb belongs to Messrs. Negretti and Zambra. We quite agree also with Mr. Scott's closing remarks. This correspondence must now cease. —ED.]

The Dutch Photographs of the Eclipse of 1871

ABOUT a year ago Dr. Schellen kindly sent me two paper copies of the Java photograph, one of them was stated to be of the size of the original negative and the other was an enlargement of about ten and a half diameters, with a delicately soft outline and much detail in the corona. On comparing this with the Indian photographs I found that though the outline of the corona corresponded depression for depression with the two Indian series, yet there was great difference in the detail of the lower parts. The question therefore arose, Was such difference to be regarded as proof of enormous change in the corona in the course of about an hour, during the passage of the totality shadow from India to Java?

I had carefully compared and catalogued the details visible upon the original negatives of the two Indian series, and had found no structure in the one that could not be traced in the other, but the details of the new Java photograph were quite of a different character, lumpy, and in more definite masses. On mentioning this to Lord Lindsay he informed me that he had other copies of the Java negatives which he had received directly from Prof. Oudemans and which were almost structureless. Mr. Davis undertook a critical comparison of the two Java photographs, and pointed out that in spite of the striking dissimilarity of the paper prints, they were evidently both taken from the same original, for they each showed a faint scratch and three minute photographic flaws in the same relative positions. It was impossible to assert that the one was a good print and the other a very bad one, for in the photograph with the delicate corona the moon's limb was soft and hazy, while with the poor corona the limb was perfectly sharp and definite. We had only one course left, and that was to infer that the softening and details had been produced artificially. Having detected manipulation in the corona, we naturally suspected it in the moon's limb, and thus arose my remark at the meeting of the Astronomical Society, that the sharp edges of the irradiation under the prominences might have been artificially produced by stopping out the moon, or rather by stopping out the hazy irradiation which presents so marked a feature, especially under the prominences in the Indian photographs, as well as in those taken in 1870.

There is still a little mystery which requires clearing up about the hazy irradiation. No trace of it is to be found in the copies of the Shelbyville photograph taken by Mr. Whipple in 1869, nor (as we now learn) in the Java photographs, although the action of the light has been greater in these than in some of the Indian and 1870 negatives, which show it as a very marked feature. We know that under ordinary circumstances hazy irradiation is produced by reflection at the hinder surface of the glass on which the photograph is taken, and that its amount may be greatly reduced by backing the plate, during its exposure, with wet paper, so as to produce a film of water instead of a film of air immediately behind the plate, thus causing nearly all the light to be transmitted instead of reflected at its back surface. Yet the Baikul photographs (and I understand also the Cadiz photograph of 1870) were backed with wet paper, and still show the irradiation very markedly.

The cause of the ellipticity of the dark moon touched upon by Prof. Oudemans seems to me to involve some very interesting questions. It is remarkable that the ellipticity does not occur in all eclipse photographs. After making allowance for the moon's motion during 40 seconds in the enlargement from the Cadiz negative, I may say that I have not been able to detect any difference between the polar and equatorial diameters in *any* of the 1870 photographs.

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